instructs the student to plot *out* from the base line (which is never done in practice) cannot be recommended.

The method of plotting adopted in practice is to calculate out from the base line to the extreme points of the survey, or to the extreme points that will appear on any one sheet of paper, and then to plot *in*. Every practical draughtsman knows that it is far easier to say, "draw a straight line" than to do it, and that an infinite amount of trouble is saved by plotting in towards small triangles from large, as the errors of the plotter are then being constantly reduced, whereas in plotting *out* they are being continually enlarged. In fact we venture to say that no one is competent to write an article on plotting who has not been in the habit of projecting surveys for no one else can understand the extreme nicety required to make three lines from three stations to the same object coincide in one point.

It is possible that Mr. Robinson has compiled this work in hopes the Admiralty may order it to be accepted as the text-book on surveying at the College. We trust, however, that their Lordships may be better advised on the point. Already we have one book, ordered to be used, which contains a theory on winds, not by any means accepted by meteorologists, and this theory has at present to be learnt by all the younger naval officers. Now we have no objection to any one theorising on wind, or any other subject, but what we do object to is that a book containing such theories should be ordered to be the standard work at the colleges, simply because the gentleman who wrote it holds, and worthily holds, a prominent position there. We think that although theories should not be absolutely excluded from textbooks, they should deal principally with well-ascertained facts, leaving the student to develop for himself a theory from those facts.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

### Natural Selection and Natural Theology

A PERUSAL of Dr. Romanes' article on Natural Selection and Natural Theology, in the *Contemporary Review* for October, 1882, suggests a few remarks upon one or two points, which may not be out of place.

One would quite agree with Dr. Romanes in "insisting on the essentially distinct character of natural science and natural theology as separate departments of human thought." True as that is, in a just sense, how does it follow that there "is no point of logical contact between" the two? Does this mean that because natural phenomena can be reduced to laws and sequences of cause and effect, no legitimate or rational inference can be made by the human mind to a *causa causarum?* It would seem so, and that it must be so to justify his very thoroughgoing conclusion: (1) That Darwin's theory explodes *particular design* (which he chooses to identify with special or independent creation); and (2) that it does not allow us rationally to introduce the conception of "an ultimate cause of a psychical kind pervading all nature," the theory having "no point of logical contact with the theory of design even in the larger sense." That is, a *raison d'être* in particular is proved to be absurd; behind all secondary causes, one such may possibly exist, but it is not to be legitimately thought of!

Or does he mean only that Darwin's theory need not, and legitimately should not, concern itself with philosophy and natural theology? Very well: then let the disciples practise what they preach, and imitate their revered master, who was content to maintain that species became what they are by descent with modification, instead of by independent creation, leaving untouched the question whether or not they were designed to be what they are. If there be "no logical contact" between Darwin's theory and the theory of design, then this renowned investigator preserved more logical consistency than some of his followers: if he refrained because of "the essentially distinct character of natural science and natural theology," and because of his determination to consider only the former, he was no less consistent.

But after all, such questions may be consistent enough, and moreover they are inevitable; and so it is not wonderful that they are raised—and not rarely prejudged—on the scientific about as freely as on the theological side.

Anyway Darwin did not prejudge the question of design, while declining to discuss it, as is done, for instance, by the *dictum* that if the species of animals and plants were slowly evolved, the evidence of design has been utterly and for ever destroyed. That has been affirmed over and over, formerly in the main by the theologians, but now, when these have seen what it comes to, mainly by the anti-theologians; by both, seemingly, under a misapprehension of the real character of the evidence for design.

Dr. Romanes' view is fairly presented in his denial that, under our present knowledge, "the facts of organic nature furnish *evidence* of design of a quality other or better than any of the facts of inorganic nature." "Or, otherwise stated, there is nothing in the theory of natural selection incompatible with the theory of theism; but neither does the former theory supply *evidence* of the latter. Now this is just what the older theory of special creation did; for it would be proof positive of intelligent design, if it could be shown that all species of plants and animals were created, that is, suddenly introduced into the complex conditions of their life; for it is quite inconceivable that any cause other than intelligence could be competent to adapt an organism to its environment *suddenly*."

Is the writer of this quite sure that any cause other than intelligence could be competent to adapt existing organisms to their environment *gradually?* How has the former presumption—the contrary of which was quite inconceivable—been done away with? For this presumption arose, and had its full force under the consideration of animals and plants produced by natural propagation; and the then irresistible inference of intelligent design was drawn directly from their adaptations in themselves and to their environment; whence it was concluded that the series of phenomena must have been instituted somehow and at some time or times (sudden creation is no doctrine of natural theology) under intelligence. How is this presumption negatived or impaired by the supposition of Darwin's theory, that the ancestors were not always like the offspring, but differed from time to time in small particulars, yet so as always to be in compatible relations to the environment? We do not see how or why the inference, which was so cogent, should under the new showing become at once irrelevant and out of all logical connection with the facts of the case, which *quoad design* are just what they were. *Suddenness*—if that must needs be entertained—is of course incompatible with the Darwinian view, and also with the facts as we understand them; but *gradualness* is in nowise incompatible with design. Under the conception of Nature as the outcome of Divine intelligence, questions of time and mode, of generality and particularity, are well nigh devoid of real significance.

But what may be contended for, and what is probably meant, is that natural selection is a rival hypothesis to design, that it accounts for all adaptations in the organic world upon known physical principles, and so renders the idea of design superfluous, as some would say; or, as it is better stated by Dr. Romanes, renders the evidence of design from these adaptations of no other or better value than that from anything else in Nature. So that the argument from teleology "must now take its stand upon the broader basis of the order of nature as a whole." This last, sensible natural theologians are prepared for. But the whole is made up of parts; and it is a whole in which the designed (if such there be) and the contingent can never be accurately discriminated, in which, indeed, from the very nature of the case, limitation is inconceivable. This need not be wondered at, since we are equally unable to discriminate the two in human

action. The evidence of design may be irresistible in cases where we cannot indicate its limits. We can only infer with greater or less probability, according to circumstances, and especially according to relation to ends. Better evidence than that of exquisite adaptation of means to ends is seldom, if ever, obtainable of human intention, and in the nature of the case it is the only kind of evidence which is scientifically available in regard to superhuman intention. Now if means and ends are predicable of inorganic nature at all, it is only by remote and indirect implication; while in organic nature the inference is direct and unavoidable. With what propriety, then, can it be affirmed that organic nature furnishes no other and no better evidence of underlying intelligence than inorganic nature? The evidence is certainly *other*, and to our thinking *better*.

To make the contrary supposition tenable, it must be shown that natural selection scientifically accounts for the adaptation; that the survival of only the very best adapted, out of the brood of more or less adapted to the environment at the time, gives sufficient scientific explanation of the adaptability or actual adaptation of the organism. Certainly this has not yet been done, and it seems incredible that it ever will be. That organisms have undergone changes as the Darwinian theory predicates, and that these changes have been picked out and led on by natural selection, seems to me most probable. That the action of the environment in some wholly unexplained way induces organisms to movement and change which would not otherwise occur, is also probable; but such change appears to be a response of the organisms to the physical surroundings and stimuli. And this most important factor in the result receives no explanation from the natural selection which operates upon it or co-operates with it. In other words, real causes have been assigned under which, *given the requisite changes*, the actual diversity and adaptations of plants and animals must or may have come to pass. But none have been assigned under which the organisms *must* have responded in the ways they do, or have responded at all, to the influences of the environment. Yet this is the very gist of the matter. The whole tenor of Darwin's writings and many explicit statements assure us that he completely recognised this distinction, which less exact minds overlook. If this distinction is valid, then the conclusion is at least premature which affirms "that the argument from teleology has been dislodged by the theory of natural selection," and its special value, as derived from adaptations in organic nature, utterly and for ever destroyed." ASA GRAY

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## Intelligence in Animals

MR. ROMANES remarks in his book that there are few recorded instances of intelligence in bears; the following facts may therefore be worth recording:—In the Clifton Zoological Gardens there are two female Polar bears between two and a half and three years old, which came here quite young. One of these shows remarkable intelligence in cracking cocoa-nuts. A nut was thrown to-day into the tank; it sank a long way, and the bear waited quietly till after some time it rose a little out of her reach. She then made a current in the water with her paw, and thus brought it within reach. This habit has already been several times noticed in Polar bears. She then took it on shore, and tried to break it by leaning her weight on it with one paw. Failing in this, she took the nut between her fore-paws, raised herself on her hind-legs to her full height, and threw the nut forwards against the bars of the den, three or four feet off. She then again leant her weight on it, hoping she had cracked it; but failed again. She then repeated the process, this time successfully. The keeper told me she employed the same method to break the leg-bone of a horse. That this is the result of individual experience, and not of instinct, is clear from the fact that her companion has not learnt the trick of opening them thus, nor could this one do it when she first came. The method of throwing is precisely similar to that adopted by the Cebus monkey described by Mr. Romanes. J. G. GRENFELL

Clifton College, Clifton, Bristol, January 15

## On a Relation existing between the Latent Heats, Specific Heats, and Relative Volumes of Volatile Bodies

AS I do not find that the following relation between the latent heat of evaporation, the expansion undergone in changing into the gaseous state, and the specific heat of a volatile body has been previously pointed out; and as, if verified, it might be of some value in the determination of one or other of the above quantities I submit it, not however without considerable diffidence, to the readers of NATURE.

Briefly stated the relation stands thus—

The latent heat of gasification at constant pressure of any body, divided by the product of the relative volume of the gas and the specific heat of the body is approximately constant; or, if

$\lambda$ = latent heat of gasification of any body,

$v$ = relative volume of the gas; *i.e.* the vol. of the body on assuming the gaseous state compared with its vol. as a liquid,

$s$ = specific heat of the body. Then

$$\frac{\lambda}{v \times s} = \text{const.}$$

The calculated value of this constant approximates to 0·8, as will be seen in the following table.

The letters $\lambda$, $v$, and $s$, heading columns 2, 3, and 4, have the same signification as above.

| | $\lambda$. | $v$. | $s$. | $\frac{\lambda}{v \times s}$ |
|---|---|---|---|---|
| Ether | 91·11 | 228 | ·515 | ·775 |
| Carbon disulphide | 86·67 | 414 | ·235 | ·890 |
| Wood spirit | 263·7 | 651 | ·645 | ·628 |
| Bromine | 45·9 | 510 | ·107 | ·841 |
| Oil of turpentine | 68·73 | 204 | ·410 | ·837 |
| Formic acid | 169·0 | 548 | ·536 | ·575 |
| Ethyl acetate | 92·6 | 209 | ·527 | ·840 |
| Methyl acetate | 110·2 | 321 | ·507 | ·677 |
| Butyric acid | 114·67 | 271 | ·503 | ·841 |
| Ethyl formate | 105·3 | 241 | ·513 | ·851 |
| Amyl alcohol | 121·0 | 268 | ·587 | ·967 |
| Acetone | 129·7 | 339 | ·530 | ·736 |
| Alcohol | 208·0 | 456 | ·547 | ·833 |
| Benzene | 91·47 | 282 | ·395 | ·821 |
| Chloroform | 61·0 | 318 | ·232 | ·828 |
| Perchloride of carbon. | 47·0 | 263 | ·198 | ·902 |
| Phosphorus trichloride | 51.42 | 311 | ·209 | ·790 |
| Methyl butyrate | 87·33 | 273 | ·487 | ·657 |
| Ethyl chloride | 93·0 | 320 | ·427 | ·679 |
| Ethyl iodide | 46·87 | 317 | ·162 | ·914 |
| Acetic acid | 102·0 | 515 | ·503[1] | ·393 |
| Chloride of arsenic | 46·5 | 324 | ·176 | ·813 |
| Tetrachloride of tin | 30·53 | 237 | ·148 | ·869 |
| Water | 537·0 | 1612 | 1·000 | ·333 |

It would appear then that the latent heat of a body may be considered as approximately proportional to the expansion of the body in vaporising and to its specific heat; and that the amount of heat required to convert a unit mass of the body at the boiling point from the liquid to the gaseous state, is equal to an amount of heat which would raise through one degree a quantity of the body in the liquid state which is approximately proportional to the expansion undergone by the liquid on evaporating.

It will be noticed that among the bodies instanced in the table there are some which appear to be very far indeed from according with the relationship in question. Notably acetic acid and water; of these, however, water presents so many peculiarities that perhaps it may be allowable to consider this as only adding one more to their number. In the case of acetic acid it is noteworthy that in plotting the curve of the latent heats of the group of acids of which acetic acid is a member, Favre and Silbermann found an irregularity arising from this body. It is, at any rate, possible that this irregularity may mean an error in the determination of the latent heat of this body.

Considering the difficulties which attend the accurate determination of latent heats, relative volumes, and specific heats of the several bodies, and that, of course, an error in any one of these will introduce inaccuracies into the constant, it may well be supposed that some, at least, of the variations noticeable in the results tabulated arise from inaccurate data. Further, there are in many cases two or more distinct determinations of these physical properties extant, of which one might be so selected in each case as to reduce the variations occurring in the constant to a minimum. F. TROUTON

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## The Gresham Funds

IN an account of a meeting of the Common Council of the City of London, held last week, I read in the *Times* that the

[1] Varies with temperature of determination irregularly.